Wm. James

THE STUDY OF

TRANCE, MUSCLE-READING

AND

ALLIED NERVOUS PHENOMENA IN EUROPE AND AMERICA,

WITH A LETTER ON THE

MORAL CHARACTER OF TRANCE SUBJECTS,

AND A

DEFENCE OF DR. CHARCOT.

BY

GEORGE M. BEARD, A.M., M.D.,

Fellow of the New York Academy of Medicine; Member of the American Neurological Association; of the New York Neurological Society; of the American Academy of Medicine; Author of Neurasthenia, American Nervousness, etc.

NEW YORK:

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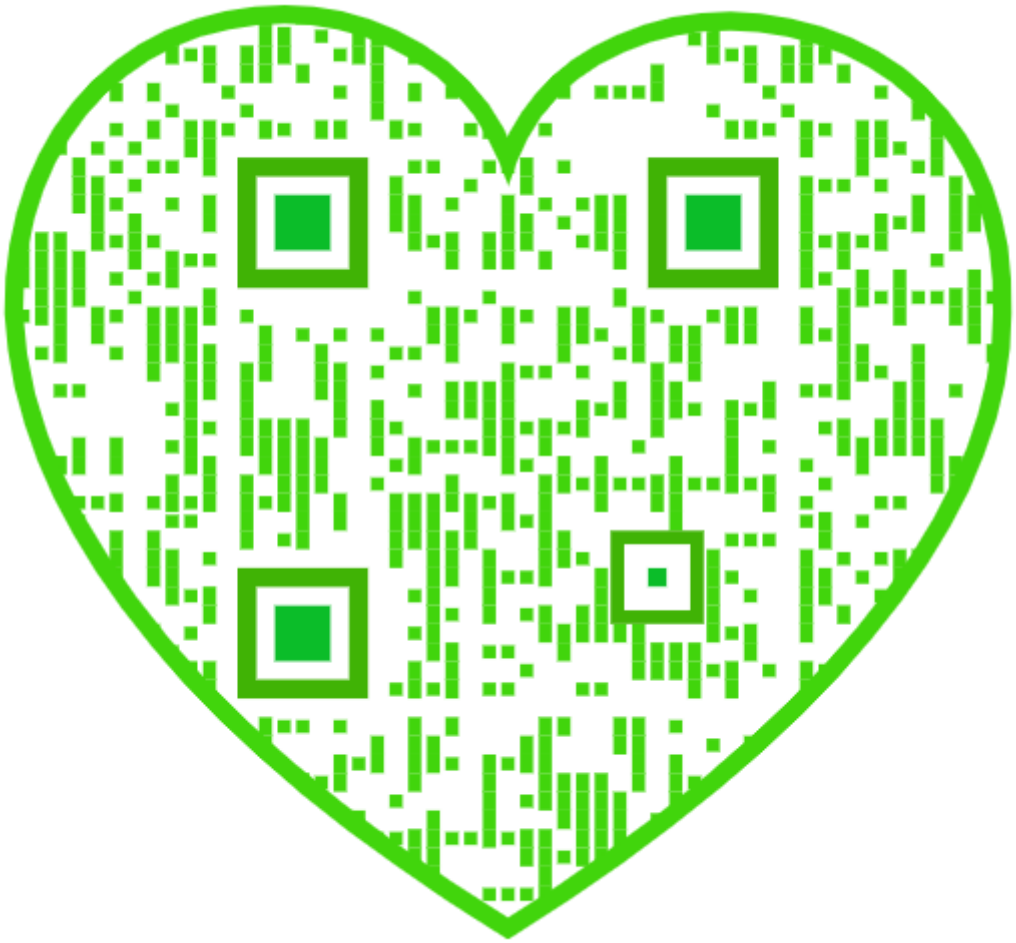

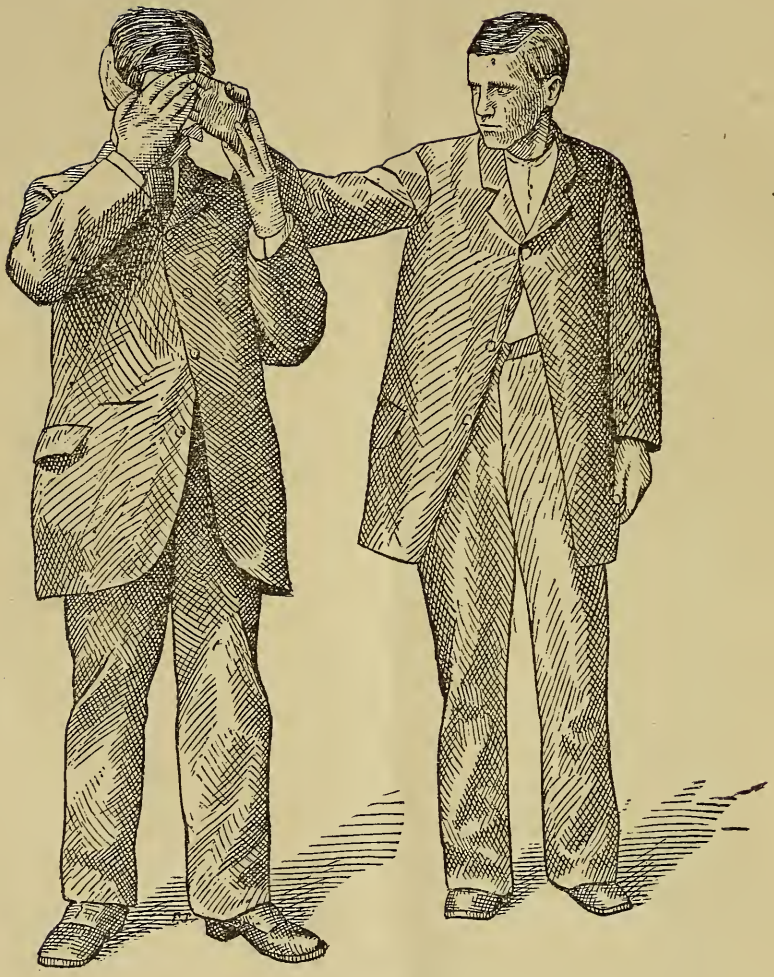

MUSCLE-READING.

(The method of "Brown, the Mind-Reader.")

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PREFACE.

THE following narratives and analyses of facts relating to the investigation of important and interesting phenomena of the nervous system are published in the belief that they will be new, in whole or in part, to the majority of neurologists and practitioners, both in Europe and America.

Some of the facts connected with the detailed history of muscle-reading are here put on permanent record for the first time, and have been obtained partly from memory and partly from public and private documents in my possession.

It is interesting and incredible that these phenomena of the involuntary life which, during the past year, have been exciting so much inquiry in the neurological world, even so recently as 1874 were almost entirely without an audience.

It is just to remind students of these topics, who are comparing the earlier and later researches, that all the leading problems connected with the nature and phenomena of Trance were solved and the whole subject organized in a science by Mr. Braid and myself, years before the recent French and German experiments were inaugurated. My own special task was to take up the subject where Mr. Braid left it and bring it out of the territorial into the organized stage. This task was substantially accomplished in the monograph published in 1877, and which is referred to in the following statement of facts.

GEORGE M. BEARD.

52 West Thirty-Fourth Street,
NEW YORK, Jan. 20, 1882.

THE

STUDY OF TRANCE,

MUSCLE-READING, AND ALLIED NERVOUS PHENOMENA, IN EUROPE AND AMERICA.

THE following résumé of what has been done in Europe in this department of psychology during the past two or three years, and a comparison of American and European researches, may be of interest to students of this side of the nervous system:

First—There has been a new departure and a positive advance in the study of Trance, Muscle-Reading and allied nervous phenomena in psychological Europe during the past two or three years. This advance consists not so much in original discoveries as in the confirmation of original discoveries previously made, and the diffusion through the ranks of psychologists in all countries of a knowledge of this subject.

The literature of any department of science is, on the whole, a good measure both of its progress and of the interest that it inspires.

I have before me a monograph, by Dr. Paul Julius Möbius, of Leipsic, "*Ueber den Hypnotismus*," in which he names ninety-two monographs and papers, under various titles, in Europe and America; and of these over two-thirds have been published within the past three years, and yet the list is far indeed from being exhaustive, espe-

cially of the older literature. Some of the treatises on the subject have never fallen under his eye; and he gives but slight attention to the very interesting literature of the 16th and 17th centuries. But, on the whole, this little pamphlet is the ablest and most philosophical analysis of this subject that has ever appeared in Europe.

Dr. Möbius is, indeed, the first European physician to look over the whole field and really understand, up to a certain point, the nature and phenomena of Trance.

The monograph of Prof. Bäumler is, however, philosophical and able, and in advance of the majority of contemporaries on this subject. Prof. Charcot said to me in 1879, that he did not profess to explain the phenomena. My claim is that the phenomena can be explained and were explained, in general and in detail, years ago.

I have before me, also, an essay, by Dr. E. Chambard, on the same subject, in which he mentions forty-one names of authors who, at different times, have treated on this subject. This interest in this realm of psychology is deepening and extending every hour; nearly all journals devoted to nervous diseases, as well as journals of general medicine, contain articles on some of the phases of this branch of science constantly. There is not agreement regarding all points that have been raised; the very abundance of the literature shows that the time has not yet come for absolute agreement; but in Germany, and, indeed, in all countries, there is a noticeable advance among those who are reading or working in these departments.

Dr. Möbius, in his very correct analysis of such of my researches as he has seen, falls into two errors which he would not have made if he had seen my first work on Trance, published in 1877.

He says very truly that the concentration theory of

trance of Schneider is in the same line with my own

CONCENTRATION THEORY OF TRANCE.

Demonstrated in my Monograph,—A New Theory of Trance.

theory. The paper of Schneider, however, which I purchased soon after it was published, did not appear for several years after the publication of my work on Trance, in which I not only suggested, but demonstrated, the truth of the concentration theory.

Dr. Möbius states correctly that the Monoideistic Theory of Braid is in the line of my concentration theory. While this is quite true, so far as it goes, yet it is also equally true that Mr. Braid never applied his theory to all the varieties of trance, natural and artificial, but merely to the artificial form which he studied. I applied the concentration theory to all the varieties of trance, natural and artificial; and claimed and demonstrated from the outset, as far back as 1876, that it unified and harmonized all the multitudinous phenomena. To sum up in a few words; the points in my own teachings on this subject, which were original and peculiar, were these three:

1st. That the artificial trance—so-called hypnotism—is but the artificial induction of the natural trance; that one principle runs through all the natural and artificial varieties; that these different varieties are not only kindred and allied, as has been hinted at by other writers, but are identical and exactly the same thing; and this I not only suggested, but demonstrated by a comparison of the phenomena. I also classified the varieties of trance.

2d. That, to induce artificial trance, there was no need of the method of Braid, or of the method of Mesmer, before Braid, or of any of the methods of the middle ages, before Mesmer; that there was no need to do anything whatever, but simply to act on the emotion of expectation—in other words, that artificial trance is a purely psychological phenomenon, and must be studied psychologically.

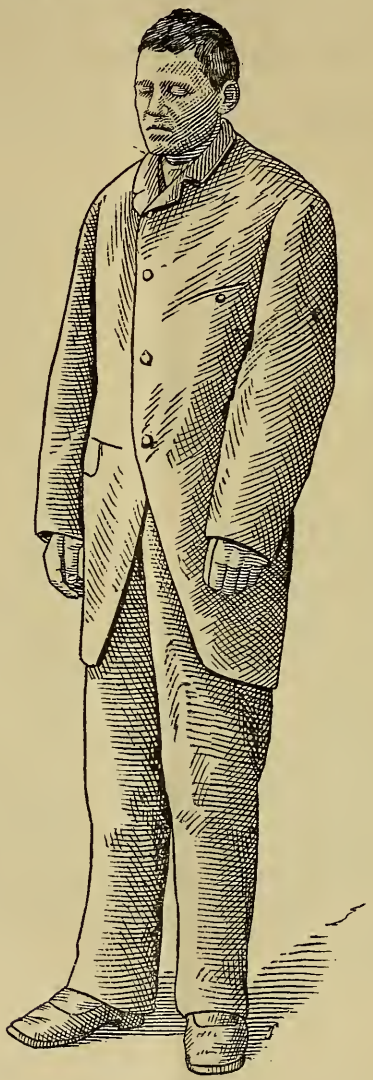
3d. That the concentration theory explained, unified

and harmonized all the varieties of trance, natural and artificial, no matter how induced; bringing order out of chaos, making clear what had hitherto been mysterious to even the most successful investigators of this side of psychology. All these principles apply to the lower animals, as well as to man; there is no distinction whatever between them.

ARTIFICIAL TRANCE.—GOING INTO TRANCE THROUGH THE EMOTION OF EXPECTATION.

More recently I have brought out the distinction between the positive and negative phenomena of artificial trance, which seems not to be understood by European writers, even at the present day; and have also demon-

strated trance confessions and trance muscle-reading, as well as trance-seeing and speaking, and abolition of vertigo.



NATURAL TRANCE—TRANCE LETHARGY. This represents the famous Pennsylvania Hungarian or Pole, who was in trance lethargy for 169 days; described by me in *N. Y. Medical Record*, p. 513, 1881

Richer, in his work on Hystero-Epilepsy, page 360, falls into the same error as Möbius. He tells us that

between the years 1860 and 1878 the subject of artificial trance was not studied; it was during that period—between 1870 and 1878—that I made my researches and demonstrated these three principles above noted, namely:

(*a.*) The concentration theory of trance, including the natural and artificial varieties.

(*b.*) The fact that nothing was necessary to induce artificial trance, and

(*c.*) That the natural and artificial varieties were all one and the same thing.

These three principles are the basis of the scientific study of Trance.

Large numbers and varieties of special phenomena were also brought out by me at that time, and the whole subject discussed in its medical and legal relations in my work published in 1877; the views of which are just now beginning to be understood and accepted in Europe.

Secondly—For the first time in the history of science, this subject is now being studied mainly by *Psychologists*, to whom it properly belongs, and to whom, had it been better understood, it would long ago have been referred. While only a few psychologists are making themselves experts in this special branch, yet only those who are experts in this branch are psychologists.

There has been an evolution in the knowledge of this side of the nervous system. In the middle ages, as well as in the 16th, 17th and 18th centuries, it was referred to priests and non-scientific men; in the latter part of the 18th it was referred to eminent men of science in general; in the early and middle portions of this century it was referred to physicians; later still, in Europe, to physiologists; and within the last few years it has been referred, both in this country and in Europe, to psychologists, although but a very limited number of psychologists are

experts in this department of psychology; and the time has not yet come when general expertness in nervous conditions, such as insanity, makes one an expert in this special branch of psychology; some of the most grotesque blunders and bad reasonings have been made by psychologists, who, without any knowledge of this subject, have attempted to write upon it.

A few years since, this subject was brought before a section of the British Association for the Advancement of Science, where it led to great excitement, and brought about a discussion between Dr. William B. Carpenter, Professor Crookes, Mr. Wallace and Professor Barrett, the well-known physicist, who read the paper that brought up the discussion.

Last year (1881) the subject was so far advanced in the right direction that it was brought before the physiological section of the British Medical Association, in Cambridge, at which discussion I was present, and in which I participated. The growing interest in the theme was evidenced by the presence of a large number of the best and most prominent psychologists in Europe. This year, for the first time, I believe, in history, it was brought to its own home, the psychological section of the International Congress. From this time forth it is quite safe to predict that this special and most interesting part of psychology will be referred to psychologists—will be discussed in psychological journals and before psychological sections and societies.

Quite true it is that Professor Preyer, of Jena, is a physiologist; but his tastes are psychological, and he has made original researches in other departments of psychology than this. Among the psychologists who have and are giving thought to this special department of psychology, are Charcot, Motel, Möbius, Müller, Berger, Bourneville,

Eulenberg, Tamburini, Tuke, Chambard, Richer, Carpenter, Lasègue, Despine, Romanes, G. Groom Robertson, Francis Galton.

The London psychologists are fortunate in having a journal called *Mind*, edited by Professor G. Croom Robertson, and devoted especially to the consideration of psychological inquiries from the psychological and philosophical point of view. This is the only journal of the kind in our language. *Nature* gives a degree of attention to psychological topics; also the *Journal of Science*, and *Journal of Mental Science*.

MUSCLE-READING (*Body-Reading.*)

Muscle-Reading—a term coined by me as far back as 1874—may be defined as the power of detecting, through physical contact, very slight and delicate muscular tensions and relaxations that result from the unconscious action of mind or body.

These muscle-reading phenomena, in the rapidity, precision and variety which they exhibit in vast numbers of scientific experiments that have been made in this country during the past year, and in Europe during the past few months, although a decade ago they would not have been admitted to be possible by any psychologist, are now fully admitted by every expert in this branch of science who has given the subject prolonged experimental attention. Muscle-reading stands in a dual relation to trance, inasmuch as both the subject and operator may, through emotional excitement, be put into a trancoidal state. The psychological interest of muscle-reading does not end with itself alone; well understood, it is the key to many of the severest and most trying problems connected with the nervous system in health and disease.

The states of the nervous condition allied or nearly

related to muscle-reading are the phenomena of somnambulism, the Jumpers, the Jerkers, and, in general, the phenomena of the involuntary life, or the reactions of mind and body.

In the frontispiece there is represented a muscle-reader, whose eyes are blinded, holding the hand of the subject, whose mind is concentrated on some locality or object in the room, or in any other room. The muscle-reader, standing in that position, detects the direction in which this object or locality is by the unconscious muscular tension of the subject, exerted through his fingers. He then walks up and down the room, and when he reaches the object or locality, he detects unconscious muscular relaxation on the part of the subject, and so knows that he has reached the locality. What he has found, he does not know (the nature of the object); but simply the *locality*. A well-trained operator may find so small an object as a pea or a pin by connecting his finger with the finger of the subject.

The marvel of these phenomena is in the minuteness and precision exhibited.

Success in this experimenting requires that the subject should keep his mind fully concentrated on the object or locality that is agreed on; but it is not necessary that he should look in that direction.

The blindfolding of the muscle-reader is not necessary, but it keeps him from obtaining information in any other way than by his sense of touch.

The method represented in this cut is that which was first publicly introduced by the famous Brown, the "mind-reader," who was to this department of psychology what Mesmer was to trance.

Like Mesmer, he did marvelous things which he himself could not explain, but which have since been explained; and

so, indirectly, he has been of immense service to science. The phenomena of muscle-reading are as genuine as the phenomena of trance and are new to science, although not new to experience; but the explanations of Mesmer and the explanations of Brown were alike erroneous. The methods of making the physical connection between the subject and the muscle-reader may be varied infinitely. There may be several subjects, as well as one, all of them with their hands on the body of the muscle-reader. All of the principal methods are described in my paper on this subject in the *Popular Science Monthly*, for February, 1877, which has been republished in the London *Journal of Science*, of July, 1881.

The method of "mind-reading" without physical contact, by the ear, the operator judging by the steps of the subject, when he is near the locality thought of, is described by me in the *Popular Science Monthly* for July, 1877; Mr. Bishop is or has been using this method in London to the great amazement and amusement of the Prince of Wales and other members of the royal family.

Previously I discussed the subject before the New York Society of Neurology and Electrology, November 16, 1874, and in the *Archives of Electrology and Neurology* for November, 1874, in a paper read before the New York County Medical Society, January 12, 1875, and in the same year before the Kings County Medical Society. My paper on "The Involuntary Life"—of which muscle-reading and trance are the culmination—was published in the *Archives of Electrology and Neurology* for November, 1875, and in the same journal for May, 1875, I published a paper on "The Nature and Phenomena of Trance." The *concentration* theory of trance was first made public in a paper read before the New York Medico-Legal Society, November, 1876, which paper was published in

the *Journal of Nervous Diseases* for January, 1877, and was subsequently republished as a monograph. An abstract of this was published in *Mind* (viii. p 568), and the *Journal of Science*, London. My papers on "Experiments with Human Beings," and on the "Scientific Study of Human Testimony," were published in the *Popular Science Monthly* for May, June and July, 1878, and March and April, 1879. In a paper on "The Psychology of Spiritualism," in the *North American Review*, July, 1879, I gave a résumé of all these and related topics.

EARLY HISTORY OF MUSCLE-READING (MIND-READING). —CONTEST WITH THE YALE FACULTY.—Since the day when Mesmer entered Paris, there has been no more important or interesting delusion, psychologically considered, than that of Mind-Reading, as introduced to the world by Brown, the mind-reader. Although these two delusions arose nearly a century apart — that of Mesmer in 1781, that of Brown in 1874—yet in their rise and in their fall they illustrated the same psychological principles.

As the phenomena attributed to animal magnetism were really the phenomena of artificial trance, so the phenomena attributed to mind-reading were really the phenomena of muscle-reading. Both Mesmer and Brown were at first sincere non-experts; and the lives of both, despite their adulteration with charlatanism, were of immortal service to psychology. There is one point of contrast: mesmerism was, and is now, the amusement of the masses; but mind-reading, from beginning to end, was the scholar's delusion, the pet of professors, the idol on whose altars our leading colleges and universities made haste to cast their crowns. Yet another contrast is found in their scientific history: it has taken almost a century to explain the phe-

nomena of mesmeric trance; the explanation of muscle-reading was accomplished in less than six months.

It was the misfortune of Brown that he came into the world half a century too late. In an unscientific age, and, indeed, at any period before the birth of modern psychology and neurology, he would probably have attained a permanent as well as powerful influence, and have been, like Mesmer, the cause of an immense literature of non-expertness. But even in 1874, in this somewhat enlightened nation, his overthrow was one of the hardest of tasks. For weeks this young and unknown adventurer, without education, without a history, held the American people by the nape of the neck, controlling the press as absolutely as a Napoleon or a Czar; until the New York *Tribune* took the lead, no newspaper dared to deny his claims. Throughout all the contest, some of the strongest advocates of Brown's claims were from the ranks of our profession; but in time, after the explanation became clear, and by experiments were confirmed, all of the physicians who interested themselves in the subject took the side of science against delusion.

I first saw Brown in one of the rooms of the Sturtevant House, New York, in the month of July, 1874. He gave that evening a semi-public exhibition, and, by previous arrangement, I was to have full and fair chance to experiment with him—and he kept his pledge—and after some of the experiments had been successfully made I gave to the audience, in a very general way, the explanation. Dr. Hammond and Dr. Burge were present, and from a subsequent conversation I learned that they agreed with me.

In the autumn of the same year it was reported in the papers that a large number of the faculty of Yale College, my *Alma Mater*, had been experimenting with Brown, and that many of them, notably Profs. Brewer and Lyman,

of the scientific department, had endorsed his claims. The names of ex-President Woolsey, Professor Norton, Professor Marsh and others were associated with the experiments; but all these were not advocates of any theory, right or wrong; but the public mention of their names in connection with the experiments made it a college matter, and was interpreted as a scientific endorsement of mind-reading. Practically, however, only a very few of the faculty took the side of delusions; the majority of the best, and best known, men in the faculty were either doubtful or gave their sympathy to the right side. President Porter very wisely said and did nothing; but the following year, as it is stated, taught the explanation of the phenomena, in his lectures to the senior class.

My public contest with the faculty was held in Music Hall, New Haven, on the evening of October 22d, 1874; and was the not entirely intended result of some private correspondence with Prof. Brewer and some public criticisms. Very great importance had been attached to the fact that the scientific faculty had made experiments through two hundred feet of wire, and had obtained results that to them seemed to overthrow my explanation. This public demonstration was not to my taste, nor to the taste of some of the members of the faculty; but it was the only way by which Brown could be induced to experiment with me, and was, under protest, accepted by me. An audience of intelligent non-experts, fully 1,000 strong, assembled to witness the contest; many of the faculty, a large number of students, and the intellectual aristocracy. The preservation of order during the very prolonged series of experiments, extending until eleven o'clock, was remarkable, inasmuch as the feeling that the college was right and that I was wrong was as unanimous and as intense as is now the feeling that Guiteau should be hanged.

It was felt that I was attacking the city and the college, that New York was pitted against New Haven. A committee of nine, of which Prof. Newton was chairman, was chosen; of the other members, I chose Dr. Sanford and Dr. Lindsley, both professors in the medical department of the university, Dr. Burge, of Brooklyn, and Hon. C. M Ingersoll. The audience treated me with consideration; but their sympathy with Brown was shown by loud applause when he succeeded, and by cold silence when he failed.

The early experiments were mostly failures, for the reason mainly that the theory of muscular action had been discussed and subjects were on their guard; but in the latter part of the evening appeared wondrous, even brilliant, phenomena, far surpassing anything I had ever seen up to that time, though not equal to what I have since seen. He found a penny in an empty seat, the letter P in Palladium, although the paper was hung up almost out of reach, selected the right one out of sixteen very small vials lying on a chair, and fixed correctly the seat of imaginary pain. He also successfully operated through two persons, in physical connection by the hands. In the famous alphabet test, through which he spelled out names thought of by the subject, he happened to fail. One of his greatest successes was to march right through the aisles and the crowd to a person sitting in the audience, on whom the subject had concentrated his mind. The rapidity and precision of Brown's movements astonished me and delighted the audience. Both the operator and the subjects —some of the best known residents—became partly entranced, through emotional excitement from the success of the last hour; for trance and muscle-reading take root in the same soil. One cool and mature medical professor who had declined to be on the commit-

tee, excitedly, in substance declared that he was *conscious that he had made no unconscious muscular motion.*

The committee reported to the effect that the theory of unconscious muscular action, even if true, had not been proved by the experiments of that evening, and that the claim of mind-reading also had not been proved. Beyond this the facts did not allow them to go; for at that time I had suggested, but not demonstrated even partially, the muscular theory. The demonstration required weeks of subsequent labor. But the suggestion of the muscular theory, in a general way, had been made before. It had been made at Ann Arbor University. *Mere suggestion is not science; science is demonstration by facts and reasonings from facts.*

One purpose in the acceptance of the contest with the faculty was to pledge Brown to give me opportunity of private experiments in order to demonstrate the theory, and one of the conditions of accepting the challenge was his pledge to give me such opportunity. This pledge he afterwards violated. At that time I did not know that any other being on the planet could do what he was doing. But so confident was I of the correctness of the muscular theory, that I told the audience to go home and experiment, and they would find that some of them could do the same thing. The result was, a triumph of deductive reasoning, and in a few weeks there were in the country hundreds of mind-readers; and this fact, more than any reasoning, destroyed Brown before the public.

My subsequent experiments were made with those who trained themselves to expertness in this matter. Judge Blydenberg, of New Haven, with whom I afterwards experimented, could, by connecting his finger with the finger of the subject, find the most minute objects, even the head of a pin when set half an inch from another pin,

of a long row of pins stuck in a table. It was impossible for me, even when keeping my arm rigid, to resist his power, provided I kept my mind concentrated on the locality.

This year (1881), seven years after the contest in New Haven, and after my experiments had been recorded in my own journal *The Archives of Electrology and Neurology*, *Detroit Lancet* (Sept. 1875), and in the *Popular Science Monthly*, and had been confirmed and accepted by scientific men, the subject was revived in London, where both the expertness and the non-expertness of the Royal Society and the royal family passed through the same stages of evolution that we had passed through in New Haven and New York. The excitement was almost as violent as here; the newspapers were loaded with the letters of non-experts, containing hints and suggestions, but no demonstration; and when two or three well-known psychologists resolved to conquer the difficulties of the subject, and made themselves masters of it by cool and repeated experiments, they did so against the opposition of the leading medical journal of Great Britain.

The excitement of last spring and early summer, with many of the members of the Royal Society and of the royal family, from the Prince of Wales down, and with a number of well-known physicians and surgeons of England was over the muscle-reading phenomena of Mr. Bishop, who, as many will remember, was in this country at the time that Brown was giving his exhibitions. After a number of the physicians had attacked the subject without thoroughly studying it or reducing it to science, a number of psychologists resolved that they would not let the matter go by default, but would see what basis of truth there might be in the alleged phenomena. The experi-

ments were made at the residence of Professor G. Croom Robertson, editor of *Mind*, in connection with George J. Romanes, F.R.S., Francis Galton, F.R.S., and Prof. Ray Lankester, F.R.S.

After investigating the phenomena in a scientific way, and with perseverance, they all agreed substantially in regard to the explanation—which others in England had suggested but not demonstrated—and the report was published in *Nature* of June 23d of this year. These investigations were made by these gentlemen independently, without any knowledge or suspicion that the subject had ever before been investigated on this side of the sea.

When a copy of my paper came to the hands of Prof. Robertson, he at once published in *Nature* for July 14th, this year, the following letter:

"I have received from Dr. G. M. Beard, of New York (well known for his studies on trance and related states), a letter in reference to the experiments with Mr. Bishop, of which Mr. Romanes has given an account in *Nature*.

"Dr. Beard, writing before our experiments were carried out, mentions his own investigation, years ago, of much more remarkable performances than Mr. Bishop's, and encloses an article "On the Physiology of Mind-Reading," which he contributed to the *Popular Science Monthly* (New York) as far back as 1877.* If this article had been shorter I would fain have asked you to reprint it, giving, as it does, a far more varied record of facts than came under our observation, and a series of carefully drawn conclusions within which our conclusion falls.

"I will only say that if I had known of this article I should hardly have thought it worth while to spend time in the trial of Mr. Bishop's powers, or even had the curiosity to attend that first meeting amid the cloud of scientific witnesses.

"July 9. G. CROOM ROBERTSON."

* The paper to which Prof. Robertson alludes has since been republished in the *Journal of Science* (London), July, 1881. A partial history of the subject is given in the same journal for September, 1881.

The report of Mr. Romanes, to which Prof. Robertson refers, is worthy of careful reading and re-reading by students of psychology. It shows that the investigations, of which it gives the results, were scientifically conducted, and that those who participated in them not only experimented judiciously and observed carefully, but, what is most noteworthy and most rare, reasoned rightly from their experiments and observations. The report appears in strong contrast with the immense literature of the subject in the medical, scientific and lay journals of Europe for weeks before, and it substantially closed the discussion. This investigation was entirely an independent one; none of the experimenters were aware at the time of what had been done here years before. Dr. William B. Carpenter was probably the only scientific man in Europe who, from the outset, had been thoroughly familiar with the American researches; he had quoted from them as long ago as 1877,* and had written me in regard to them as far back as May, 1879, asking me (inasmuch as, according to his statements, I had given more attention to trance and allied subjects than he) to send him all of my writings on them—especially requesting the essays on Trance and the Involuntary Life—to assist him in completing a paper that he was preparing for *Fraser's Magazine*, for July, 1879. In this letter Dr. Carpenter expressed himself as specially gratified with my formulation of the six sources of error in experiments (1881) with human beings. These documents, including another paper on muscle-reading, were promptly sent; but Dr. Carpenter's paper, which he said he was in haste to complete, so far as I can learn, never appeared. The next year

* Mesmerism. Spiritualism, Historically and Scientifically Considered. By Dr. Wm B. Carpenter, C.B., M.D., L.L.D., F.R S. New York: D. Appleton & Co. pp. 49–50.

Prof. Huxley was invited to study these muscle-reading phenomena, in connection with Dr. Carpenter, and he accepted the invitation; but was not informed by Dr. Carpenter of the American researches. Prof. Huxley informed me, in a letter received before I left for Europe, that his name had been used in connection with the subject without his authority, and that he knew nothing about it except as above stated. Later still last year (1881) the scientific men of London in large numbers were encouraged by Dr. Carpenter—in a letter published through Mr. Bishop—to study these phenomena, and they also were not informed of the American researches; but were led to suppose that the phenomena were new to science; and thus, very fortunately, they had opportunity, which some of them enjoyed, to study the subject independently.

The experiments here referred to, by which the phenomena of muscle-reading or body-reading were originally brought into service, were many of them made, in the first instance, with a person of whose character and previous history I knew nothing; whether his name (Brown) was real I did not know, and do not know to-day, for with the experiments his character had nothing whatever to do.

Subsequently I made a series of investigations with muscle-readers whom I did know or believe to be of the highest character; but I treated them experimentally, as though they had no character at all. I trusted nothing to their honor or their statements, and I so informed them after all was over. Experiments with human beings, the results of which depend upon the real or supposed character of the subject experimented on, have never commanded, and ought never to command, the homage of science. I became absolutely convinced that one of the best of the muscle-readers on whom I experimented (Brown) was in many respects very untrustworthy, but the scientific

researches made with him were just as valuable as those made with those who were known or believed to be both intelligent and reliable.

Precisely so in England, in the recent and independent demonstrations of muscle-reading by Romanes, Robertson, Galton and Lankester. These gentlemen knew very little of the character of the individual on whom they experimented. Such knowledge was not necessary or possible for them, and they did not insist upon it in their report, the conclusions of which, as far as they go, are entirely in harmony with my own.

If Mr. Bishop had been in all respects candid he would have told them that he was in America in 1874, at the time of the muscle-reading excitement, that he knew me, and knew all about my researches in that subject quite as well as Dr. Carpenter.

The Maine Jumpers.—My researches on the Maine Jumpers, the phenomena of which are of a trancoidal nature, and probably are an evolution of tickling, have received the following confirmation, which was published in the *Journal of Science* (London) for September, 1881. The phenomena of the jerkers allied to those of the jumpers are well described by Dr. Yandell, in *Brain* for October, 1881, and *Popular Science Monthly*, February, 1882.

"JUMPERS IN SOUTH AFRICA.

"To the Editor of the Journal of Science: *Sir*—I was much pleased with the article in the *Journal of Science*, by Dr. G. M. Beard, on the 'Jumpers of Maine.' The same phenomena were observed here amongst a set of farmers living about the Divisions of Richmond and Graaf Reinet, in this colony.

"The first one I met with was a young man of about twenty years; he was then (about the year 1837) on a visit to Stellenbosch, when all the different tricks mentioned by Dr. Beard were played upon him. His rapid repetition of short sentences, when startled, was most remarkable, and quite uncontrollable by his will. In more than one

instance he would utter a whole sentence even before the last part had been expressed (?), which often annoyed him very particularly when he found that he had made use of words which were not fit to be expressed in company of ladies. He knew what was coming, but could not help himself. This young man was a Siebenberg, and the disease was principally noticed in that family.

"A couple of years after that I again came across an old man, a Mr. Charles Liebenberg, who was subject to the same disease, as also three of his sons-in-law, named Pienaar.

"Not many years ago, whilst living at Worcester (South Africa), I became acquainted with another person of the same stamp, Mr. Conradie, a resident of Graaf Reinet, who, as far as I am aware, is still alive. He would throw, strike, jump, etc., repeating the word also at the same time—whatever he was ordered to do when startled. I was present one day when some of these tricks were played on him, when he gave a young lady who was standing close to him such a violent blow as to send her spinning to the ground. He answered completely to the description given by Dr. Beard (*Journal of Science*, 1881, page 87: London).

"None of these men were deficient in intellect; the one last named is a decent, well-to-do farmer. Two of the Pienaars were, moreover, brave men. They were all very ticklish, and sometimes it was only necessary to point with the finger at them and mention a word. Both Conradie and the Pienaars are of French descent.

"I am, &c.,
"J. W. HUGO,
"*Civil Commissioner and Resident Magistrate,*
"*Clanwilliam, South Africa.*"

Professor Preyer, of Jena, had previously stated, in a note to me, that some of the phenomena of the Jumpers had been observed by him in his laboratory, in guinea-pigs, which were kept for the purpose of experiment. Similar phenomena have been observed among the Malays, as long since recorded in the London *Medical Record.*

My experiments in Trance-seeing had already been confirmed this side of the Atlantic by the well known neurol-

ogists, Dr. C. L. Dana, and Dr. W. J. Morton, of this city. Dr. J. G. Davey, of Bristol, has recorded similar experiences in the *Journal of Psychological Medicine*, vii., part 1. Dr. Davey's experiments favor the first of my hypotheses—that of the general sensation doing the work of the special sense. The claim is very old, but has been but little studied. See *Journal of Science*, September, 1881.

The relations of this whole subject to the principles of evidence—perhaps the most radical and revolutionary of all the ideas advanced here—has likewise received an independent and partial confirmation in the remarkable essay of Professor Wundt, of Leipsic, in his controversy with his brother professors, on the Slade experiments, three years ago. This essay was published in the *Popular Science Monthly* for September, 1879. I would take the liberty of asking those who are interested to compare this essay of Prof. Wundt with my series of papers on the "Scientific Study of Human Testimony," and experiments with human beings, as published, before that date, in the *Popular Science Monthly*, above referred to.

It has long been a matter of surprise that the Germans have been so slow in taking up this subject; for it is peculiarly and specially adapted to the philosophic German mind. For this neglect Germany is now beginning to atone. The study of this subject began in Germany in the last century; it is proper that it should return to its early home.

The question so often asked in all countries—What practical good comes from these researches in the phenomena of the nervous system?—was well answered in Sir James Paget's address at the opening of the International Medical Congress; and to that address—noble all through, but especially strong on that point—the value of science—I would refer those who make such inquiries. The special need at this stage is not so much of experimenting

as of thinking, of reading, reflecting, comparing and co-ordinating what has already been done in scientific Europe and America; and those who do thus read, reflect and compare will find very soon that this phase of psychology has really a practical as well as a scientific interest; that not only does it have an elevating and disciplining influence on the mind, but that its direct and inevitable tendency is to make us wiser, more skilful, and more successful in the study and treatment not only of neuroses, but of all other forms of disease.*

The following is the report of the English psychologists on the Muscle-Reading performances of Mr. Bishop. as published in *Nature*, for June 23, 1881.

The parallelism between these methods and their inferences as far as they attempted to go, and my own methods and reasonings, is very interesting, and all the more so because these researches were made independently. It is clear that Mr. Bishop is a very moderate performer as compared with Brown the mind-reader, or with Judge Blydenberg of New Haven, and others whom I have trained, or who have trained themselves to detect unconscious muscu-

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* The following extract from a late editorial in *The Lancet* may well be borne in mind by every physician: "It is time to put in an earnest plea for the recognition of psychology as an intimate part of physic. Every advance we make in the exploration of Nature renders it increasingly evident that body and mind are inalienable—as in health so in disease. The mental phenomena of life are the direct effects or the reflexes of the physical state. It is, therefore, obvious that the moment is approaching when the recognition of unity must be considered."

The direct and indirect medico-legal relations of natural and artificial trance are alone of immense interest and importance. This was so strongly impressed on my mind, that I first announced the concentration theory of trance before the New York Medico-Legal Society; and since that time other papers on the same subject have been read before the same society, by Dr. Hammond, Dr. Crothers and myself.

In two of the most remarkable and important trials of the century—that of Cadet Whittaker and that of Guiteau—trance phenomena have been introduced—in the first as a positive defence, and in the second as an incidental part of the prosecution.

lar tension or relaxation. Reasoning deductively from the established principle that the general sensation is variously subdivided and specialized, I concluded from the outset of my studies of this subject that the esthesiometer would be of no service in solving the problem why some persons can detect these muscular thrills or pulses, while others cannot; the experiment of these psychologists confirm, so far as a single case can, that deduction.

Moreover, the terms "dreamy abstraction" or "reverie" really mean a trancoidal state into which both subject and operator are liable to be thrown through excitement of the emotion of wonder. Hence the experiments with instruments like the esthesiometer should be made on the operator after he is entranced, and not when he is in the normal condition. But even these might teach us very little. In two instances in this country performers in these experiments went into convulsions.

"THOUGHT-READING.

"The public mind has been of late somewhat agitated by the doings of a Mr. Bishop, who has come before the world of London society in a capacity no less startling than that of a professed reader of thought. Armed with a favorable letter of introduction from Dr. W. B. Carpenter, he has not only taken by storm the general public and daily press, but also succceded in convening an assembly of scientific men to witness his performance, which in point of numbers and importance resembled in miniature a *soiree* of the Royal Society, while still more recently he has had the honor of exhibiting his powers before the Heir Apparent to the Crown. There is no doubt that Mr. Bishop owes this wide and sudden celebrity to the patronage which was extended to him by the great opponent of all humbug; and although Dr. Carpenter doubtless intended his letter to exert a salutary influence by recommending Mr. Bishop to the attention of the credulous, it is to be regretted that it served to recommend him also to the attention of the scientific. This is to be regretted, because the result was to endow the powers which were afterwards exhibited with a fictitious degree of

importance in the eyes of the public, and also to bring a large number of distinguished men into the somewhat undignified position of acting the stalking horse to Mr. Bishop's notoriety. But however this may be, it seemed to Prof. Croom Robertson worth while to make a more careful trial of Mr. Bishop's powers than was possible in the first crowded assembly, and he therefore invited Mr. Francis Galton, Prof. E. R. Lankester, and myself, who were all present on the first occasion, to join him in an investigation. When we had assented to the proposal, Mr. Bishop was invited to meet us at Prof. Croom Robertson's house. He immediately accepted the invitation, and it is but just to state that throughout the investigation which followed he placed himself entirely in our hands, and with the utmost good nature submitted to all our requirements. He professes that he is himself ignorant of his *modus operandi*, and merely desires that this should be adequately investigated and satisfactorily explained.

"Two meetings were arranged. At the first, which was held on May 28, Prof. Lankester was not able to attend, and his place was taken by Mr. Leslie Stephen. Mr. Alfred Sidgwick was also present. At the second meeting, held on June 11, there were present as before, Prof. Croom Robertson, Mr. F. Galton, and myself, but Mr. Leslie Stephen and Mr. Alfred Sidgwick were absent, while Prof. Lankester was present. The room in which both meetings were held was a double drawing-room of the ordinary shape of those which usually have folding-doors; here however the folding-doors were absent. The extreme length of the room was 36 feet, the width of its front part was 19 feet, and of its back part 12 feet.

"First, Mr. Bishop was taken out of the room by me to the hall down stairs, where I blindfolded him with a handkerchief; and, in order to do so securely, I thrust pieces of cotton-wool beneath the handkerchief below the eyes. In all the subsequent experiments Mr. Bishop was blindfolded, and in the same manner. While I was doing this, Mr. Sidgwick was hiding a small object beneath one of the several rugs in the drawing-room; it having been previously arranged that he was to choose any object he liked for this purpose, and to conceal it in any part of the drawing room which his fancy might select. When he had done this the drawing-room door was opened and the word 'Ready' called. I then led Mr. Bishop up stairs, and handed him over to Mr. Sidgwick, who at that moment was standing in the middle line between the two drawing-rooms, with his back to the rug in ques-

tion, and at a distance from it of about 15 feet. Mr. Bishop then took the left hand of Mr. Sidgwick, placed it on his (Mr. Bishop's) forehead, and requested him to think continuously of the place where the object was concealed. After standing motionless for about ten seconds Mr. Bishop suddenly faced round, walked briskly with Mr. Sidgwick in a direct line to the rug, stooped down, raised the corner of the rug, and picked up the object. In doing all this there was not the slightest hesitation, so that to all appearance it seemed as if Mr. Bishop knew as well as Mr. Sidgwick the precise spot where the object was lying.

"This is Mr. Bishop's favorite experiment; so I may give some of our other observations relating to it before passing on to the variations which we introduced. It was soon found that he succeeded much better with some of us than with others; so at the second meeting, in order to make a numerical comparison, he was requested to try two experiments with each of the four persons who were present. With Mr. Galton, Prof. Robertson and Prof. Lankester he failed utterly, while with myself he succeeded once perfectly, and the second time approximately. For, on the first occasion, I concealed a pocket match-box upon the top of a book, behind the leather lap of a book-shelf. After feeling along the rows of books for some time, he drew out the one on which the match-box was lying. In the second experiment, I placed a visiting-card on the key-board of a grand piano, and closed the cover. After going about the room in various directions for a considerable time, he eventually localized the piano, and brought his finger to rest upon its upper surface about six inches from the place where the card was lying. It will thus be seen that his success with me, although so much better than with any of the other three persons present that evening, was not so immediate and precise as it had been with Mr. Sedgwick the evening before. It has also to be mentioned that in one of the experiments which he tried with Prof. Robertson the evening before, he was, after a good deal of feeling about, successful in localizing a particular spot on an ordinary chair which Prof. Robertson had selected as the spot to be found. From this it will be seen that it made no difference whether a particular article or a particular spot was thought of; for if the subject thought of was a certain square inch of surface upon any table, chair, or other object in the room, Mr. Bishop, in his successful experiments, would place his finger upon that spot. Neither did it make any difference whether the article or place thought of was at a high or a low elevation. Thus, for instance, in one of the experi-

ments, I placed a small pencil-case high up in the chandelier of one of the drawing-rooms. There was first a great deal of walking about in various directions, examining tables, book-shelves, etc., so that it was thought that the experiment was about to prove a failure. (It may here be mentioned parenthetically that in all the experiments tracings were taken of the routes which Mr. Bishop traversed, but it seems needless to occupy space with recording the analysis of these results.) Then, while feeling over the surface of a table in the other drawing room, and not far from the corresponding chandelier, Mr. Bishop suddenly pointed at arm's length vertically to the ceiling. He remained motionless in this position for a few seconds, and then set off at a brisk pace in a straight line to the other drawing-room, until he came beneath the other chandelier. As his finger was all this time pointing to the ceiling, it touched this chandelier on his coming beneath it. He then stopped and pointed as high as he could, but not being a tall man, was not able to touch the pencil-case, which had been purposely placed above his reach. After satisfying ourselves that his determination to reach up at that particular spot could not be attributed to accident, but rather that his finger appeared to be smelling the object of his search, the experiment was concluded. As a rule, unless success is achieved within the first two or three minutes, it is never achieved at all; but in some cases, as in the one just quoted, after several minutes of feeling about in various places and directions, a new point of departure seems suddenly to be taken, and Mr. Bishop starts off straight to the right spot. As an instance of this, I may quote another experiment, in which I placed a shilling beneath a sheet of paper lying on a table which was crowded with other articles. After going about the room in various directions for a considerable time, this table was reached, apparently by accident, and just at the time when I was thinking that the experiment would certainly prove a failure, Mr. Bishop suddenly became more animated in his movements, and exclaiming, 'Now I am within two feet of it,' began to hover the point of his finger over the table, and eventually brought it down upon the sheet of paper just where the shilling was lying beneath.

"Mr. Bishop can also very frequently localize any spot on his subject's person of which the subject may choose to think. As in all other cases, he presses the hand of the subject upon his forehead with one hand, and uses the other as a feeler. Here, again, he succeeds much better with some persons than with others, and the persons with

whom he succeeds best are the same as those with whom he does so in his other experiments. Thus, he altogether failed with Mr. Galton, although the latter, in order to fasten his attention the more exclusively on one particular spot, pricked this spot with a needle. With Prof. Lankester success was partial; for while he thought of the point of his nose, Mr. Bishop was only able to say that the point thought of seemed to occupy the median line of the body on the front aspect. But on a previous occasion, at Bedford Square, Mr Bishop localized correctly a pain (slight toothache) from which Prof. Lankester was suffering. With Prof. Croom Robertson success was better, though not quite perfect, for while the place thought of was the ball of the right thumb, Mr. Bishop localized it in the right wrist. In the only two experiments tried in this connection with myself, the results were somewhat peculiar. In the first experiment, I thought of a spot situated under the left scapula, and Mr. Bishop localized it as situated under the right; in the second experiment, I thought of my right great toe-nail, and for a long time Mr. Bishop prodded round and on the left great toe-nail, though he eventually changed to the right one, and so localized the spot correctly. In both these experiments, therefore, it seemed that with me Mr. Bishop experienced a strong tendency to confuse symmetrically homologous parts.

"From this brief summary of the results gained by following Mr. Bishop's own methods, it will be seen that on the whole his power of localizing objects or places thought of by a person whose hand he clasps is unquestionably very striking. Of course, the hypothesis which immediately suggests itself to explain the *modus operandi* is that Mr. Bishop is guided by the indications unconsciously given through the muscles of his subject—differential pressure playing the part of the words 'hot' and 'cold' in the childish game which these words signify. Mr. Bishop is not himself averse to this hypothesis, but insists that if it is the true one he does not act upon it consciously. He describes his own feelings as those of a dreamy abstraction or 'reverie,' and his finding a concealed object, etc, as due to an 'impression borne in' upon him. But however this may be (and of course we had no means of testing the statement) all our experiments have gone to show that the hypothesis in question is the true one, and that Mr. Bishop owes his success entirely to a process of interpreting, whether consciously or unconsciously, the indications involuntarily and unwittingly supplied to him by the muscles of his subjects. Thus when his

subject is blindfolded and loses his bearings, failure results. Failure also results if the connection between Mr. Bishop and his subject is not of a rigid nature—a loose strap, for instance, being apparently of no such use to him for the establishment of connection as a walking stick. Similarly, although he was very successful when he grasped my left hand when I did not know where the object was concealed, but when my left wrist was held by Mr. Sidgwick, who had concealed the object; he failed when, under otherwise similar circumstances, Mr. Sidgwick held my right hand—so establishing a limp instead of a firm connection through my person.

"Lastly, a number of other experiments were tried, in deference to some statements which Mr. Bishop made concerning his occasional success in reading thoughts of a kind which could not be indicated by muscular contraction. From these experiments, it is needless to say, we did not anticipate any results; but (with the exception of Prof. Lankester) we thought it was worth while to make them, not only because Mr. Bishop seemed to desire it, but also to satisfy the general public that we had given the hypothesis of 'thought-reading,' as well as that of 'muscle-reading,' a fair trial. The experiments consisted in the subject looking at some letter of the alphabet which Mr. Bishop could not see, and the latter endeavoring to read in the thoughts of the former what the letter was. Although this experiment succeeded the first time it was tried, it afterwards failed so frequently that we entertain no doubt as to the one success having been due to accident, and therefore conclude that if Mr. Bishop has any powers of 'thought-reading,' properly so-called, he has failed to show us evidence of the fact.

"Deeming it a remarkable thing that such precise information as to a mental picture of locality should be communicated so instantaneously by unconscious muscular movement, we thought it desirable to ascertain whether Mr. Bishop, who is able so well to interpret these indications, is endowed with any unusual degree of tactile sensibility or power of distinguishing between small variations of resistance and pressure. We therefore tried the sensitiveness of his finger-tips with the ordinary test of compass-points, but found that he did not display more than a usual delicacy of tactile perception, while his power of distinguishing between slight differences in weights placed successively on a letter balance concealed from his eyes was conspicuously less than that displayed by Prof. Croom Robertson. As Mr. Bishop is not op-

posed to the hypothesis by which we conclude that his results are obtained, there is no reason to suppose that he tried to depreciate his powers of tactile sensibility and of distinguishing between small differences of weight. In their main features Mr. Bishop's experiments are frequently performed as an ordinary drawing-room amusement, and we are therefore inclined to think that he does not enjoy any peculiar advantages over other persons in regard to sensitiveness of touch or power of appreciating pressure, but that his superior success in performing the experiments is to be ascribed merely to his having paid greater attention to the subject.

"In conclusion, we desire to express our thanks to Mr. Bishop for the trouble which he has taken in submitting to the numerous experiments, the general results of which have now been stated.

"This report has been read in proof by Prof. Croom Robertson, Mr. Francis Galton, and Prof. E. R. Lankester, and meets with their full approval.

"GEORGE J. ROMANES."

The following letter appeared in the New York *Medical Record* for January 21, 1882. It may be read in connection with my letter in the London *Times* for August 11, 1881, and also in connection with the letter of Dr. Hammond* in the New York *Medical Record*, October 15, 1881, and with my paper on "Current Delusions Relating to Hypnotism," in *Alienist and Neurologist*, Jan. 1882.

Facts relating to unfamiliar truths in science cannot be repeated too often, or under too many forms. *I may, therefore, state once more that the trained subject I took to*

* Dr. Hammond's letter, leaving out the kindly and complimentary references to myself, has a permanent value as a clear and scientific statement of some of the principles that lie at the basis of experiments with human beings. The very old and dying delusion that the moral character of trance subjects should be a factor in determining the results of experiments kept back this department of science for nearly a century.

The defects of the Baconian philosophy in its application to living human beings were specially impressed upon me while making the muscle-reading experiments. At that time I formulated the *six sources of error* as subsequently presented before the American Neurological Association, published in *Popular Science Monthly*, March and April, 1879, and analyzed in *Mind*, July, 1879.

London, after months of experiments with him in this country, was not only honest but extraordinary; and some of the phenomena of artificial trance exhibited there, as well as here, were among the most original and remarkable ever witnessed in Europe or America; and will so be understood, and in less time than has been necessary for the confirmation of these researches in muscle-reading, the Jumpers, Human Testimony, and other phenomena of the nervous system.

"THE MORAL CHARACTER OF TRANCE SUBJECTS.

"A DEFENCE OF DR. CHARCOT.

"To the Editor of the *Medical Record*.

"Sir: It is a rule of my life, from which I rarely depart, to hold no public discussion on scientific subjects with non-experts. The excellent and amiable Englishmen, authors of the recent letters in regard to my experiments in Europe, must therefore look to other sources for answers to their very amusing inquiries.

"I feel it, however, to be my duty to say a few words against the extraordinary, though unintentional, attack made upon Dr. Charcot by Dr. Bucknill. The exact language of Dr. Bucknill is: 'I should be indeed surprised if the accomplished physician (Dr. Charcot) who was investigating them had been willing to eliminate the moral character of each patient, as no factor in the case.' If there is any truth in this charge, then Dr. Charcot is both a non-expert and a fraud; for he has both publicly and privately contended for the genuineness of his trance experiments, on the same ground that Dr. Hammond and I do, namely: *his own private study of the phenomena by numerous and repeated tests, in which all the sources of error have been eliminated.*

"Such was his position when, three years ago, he showed me his cases, allowed me to experiment with one of them, and discussed this very question; such has been his position in his public lectures; and such is the position in Richer's work on 'Hystero-Epilepsy' (pp. 579 et seq.), which these trance researches make an important feature. Such, also, was his position but a few weeks since, when he exhibited his cases to one of our American physicians; and such is the position which in future all who aim to know anything of this subject will take,

and without argument. *The physician who, at this late hour, after all the revolutionary advance that has been made in the study of human beings, inquires into the moral character of those on whom he experiments, however wise and mature he may be on other themes, is on this theme but a child—as much behind the age as he who should use inoculation instead of vaccination, and he should wait to grow before he expresses in public any opinion on either side.*

"It is well known, and has been publicly stated, that the individuals on whom Dr. Charcot experiments would scarcely be acceptable candidates for teachers in Sunday-school; but, whether these special charges be true or not, it is to the glory of Charcot—for which he will be remembered long after his other labors and the names of his non-expert opposers are forgotten—that he has taken these emotional and wretched women, these waste products thrown off in the evolution of the race, and has so experimented with them as to produce results as clear, as true, and as pure as the chemist can obtain with the inanimate elements in the laboratory.

"Serious mistakes of inference Dr. Charcot has certainly made, and is making now; and for these no one has criticized him more earnestly or in a more public manner than I have; but none the less do I respect him as a man of genius and a man of honor, who does not deceive, but tells the exact truth when he says that the genuineness of trance phenomena, as studied by him, is established by *his private experiments.*

"*If he does depend on the character and history of his subjects, and should so declare and confess, then his experiments are not worth exhibiting to scientific men, or publishing to the scientific world.*

"The worst cases of sham trance I have ever seen have been in men and women whose moral character and position have been in other respects unimpeached. If we do not know whether the phenomena are genuine before we inquire as to the character of the subject, we shall not know after, whatever the result of the inquiry may be.

"I would take this occasion, in reply to inquiries that have been privately made by those who are taking pains to inform themselves in regard to this side of the nervous system, that of the general honesty of my trained subject, I know and care nothing; *of his honesty in relation to the trance experiments*, however, I have better proof than I have of the honesty of any one who witnessed the experiments in Europe or America; that, on the average, he did quite as well there as here; sometimes succeeding, sometimes failing, sometimes succeeding in part

only, as is often the case with genuine subjects; that he never did better than in some of his experiments on the day that has given rise to the discussion; that, with perhaps three or four exceptions, he was the best subject I have ever seen—among the nearest approaches being the thoroughly trained subjects of Charcot, which, however, are trained *psychologically*, mainly by the method of Braid, in which expectation is only one factor, while mine are trained *psychologically*, wholly through the emotion of expectation; that the experiments were received—on the part of those who are making themselves experts in this direction—with the highest conceivable interest, surpassing that which has been accorded to any subject that I have had the pleasure of bringing to the attention of physicians in Europe, either in this year or in previous years. While some of the phenomena were old, others were in detail comparatively new discoveries, and, as such, were appreciated. After those who are studying these topics had opportunity for private examination of the case, a portion of a day was devoted entirely to non-experts, who were invited for the same reason that they were invited here, and in the expectation that some of them might be inspired to begin the investigation of this side of the human system for themselves, as indeed has been the case in America and Europe.

"I would here defend England against herself, or rather against some of her representatives. With one or two exceptions, the distinguished and able medical gentlemen who witnessed the experiments on the non-experts' day, were gentlemanly, considerate, and attentive; the arguments, queries, doubts, suspicions, and difficulties that were raised among them were precisely those that have been gone over and over again by scores of American physicians in my office, and in public experiments in America. Non-expertness, indeed—like small-pox—has the same symptoms in all countries. We cannot expect children to reason like adults; and we cannot hope that non-experts will see with expert eyes, but must give them, little by little, the alphabet of science, and not become impatient if, like children, they ask questions that are elementary and imperfect, and our answers are not understood. We are not to ask them to apologize, but simply wait for them to mature; an infant class in psychology is not to be judged by the standard of disciplined philosophers. Merely seeing trance experiments gives one no more knowledge of trance than seeing the captain of a ship taking observations makes one a navigator. Dr. Bucknill's con-

clusion, from witnessing Dr. Elliotson's experiments, that some of them were genuine, is as worthless as his conclusion that some of them were simulated. Even one who knows something of this subject—even the very highest expert—may not be able to tell from seeing these experiments whether they are or are not genuine.

"The trained subject used in London is now in this city, and any one interested can have the opportunity of convincing himself by private experiment of his honesty.

"I have been many times interested and aided by witnessing experiments and operations in departments in which I was an utter non-expert, since I have been stimulated thereby to a study of the subject. It is not an insult, but a compliment to a man to call him a non-expert; for the term implies that there may be some subject on which he is an expert.

"In England as in America, and everywhere, the difficulty in these and allied matters is not so much moral as intellectual; our critics are oftentimes as sincere as ourselves, and quite as earnest, doing their best duty according to the light that God gives them; gallant, generous, and noble natures oftentimes, whose friendship is a possession, but who —from long living in an atmosphere that is a non-conductor for new ideas—become antidotes to science rather than its adjuvants; they prefer to be islands, standing still in the midst of the stream, than ships moving with it; though they would tread softly on a worm, they would banish genius from the universe.

"It was not viciousness, but simply Anglo-Saxon non-expertness—a special and scientific term for what Mr. Matthew Arnold calls 'Philistinism'—which, stimulated by conscience and armed with power, persecuted Jenner, and now, with yearly increasing strength in Europe, opposes vaccination; forced Ferrier into the police court, and, under sanction of a most cruel law, which even medical men and medical journals allowed themselves to approve, drives from Great Britain its best men of science as the Huguenots were driven from France; declined a paper from Mr. Braid on some of the most scientific researches of the century, and then, forty years later, must send to Germany for Professor Preyer to explain what Mr. Braid had vainly sought opportunity to explain; whereupon hours are spent in praising that same Mr. Braid, without clearly knowing either what he did or did not do for science; which, in our own country, refused permission to Dr. J. K. Mitchell to read an essay so classical that—despite its errors—the

experts of the future must quote it; ordered Dr. Clymer and his friends to stop investigating science, or stop practising medicine; and resolutely snubbed Dr. Austin Flint when he wished to present to the American Medical Association the strongest and most original contribution he has ever made, which Professor Schiff confirmed and the French Academy gladly received

"The advantage of having some knowledge of the subject of which we write and speak, even in matters of science, is so great, that one is amazed to hear a non-expert speak on this subject at all, save with muffled lips and bated breath; but it is the very nature of non-expertness not to know that it does not know, and in dealing with experts to mistake calmness for weakness, and silence for consent to delusions.

"In closing, I would congratulate experts, partial experts, and experts in the making, of both Europe and America, on the auspiciousness of the time in which they enter upon and continue their studies. As a result of the efforts of many pioneers, the study of these phenomena of the nervous system has at last been made both fruitful and safe. Before I reached England this year, some of the most important and incredible of my experiments in this department of science had been confirmed by a number of well-known English psychologists, and, in the handsomest way possible, acknowledgments of priority had been publicly given in their leading scientific journals. The details, as gathered from *Nature*, will soon be republished in America. The excellent paper and experiments of Dr. Mills, before the Medical Society of Philadelphia, and the intelligent discussion thereof, as reported in the *Medical Times* of November 19th, would have been impossible five years ago, and would have been impossible to-day, except in America and Germany.

"Observe the normal evolution of our ideas on this theme: The long and dreary stage of cold indifference has passed already; the short, hot stage of opposition is rapidly passing; the third and last stage, through which all scientific truth must pass—the stage of contests of priority—is near at hand. For the discussion of this the highest expertness of both hemispheres is invited, and, as I predict, will soon be enlisted.

"GEORGE M. BEARD.

"NEW YORK, 1881."